INTELLIGENCE
AND
FERTILITY

*THE EFFECT OF THE DIFFERENTIAL
BIRTHRATE ON INBORN MENTAL
CHARACTERISTICS*

BY

SIR CYRIL BURT

The Eugenics Society

and

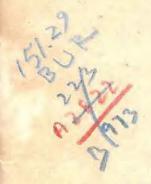CASSELL AND COMPANY LTD

LONDON

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PREFACE

THE request for a second edition of this pamphlet has made it possible to correct a few slips and ambiguities in the text and to add a short appendix dealing, so far as space allows, with recent criticisms and further inquiries.

A detailed answer to all the objections raised by different critics is scarcely possible here. A few, I hope, will be removed by the corrections made and the footnotes added in the text. A fuller discussion of the remaining points will be found in the articles cited in the appendix.

The following pages were originally drafted as a memorandum to the Royal Commission on Population; and their main purpose was to urge the need for a succession of psychological surveys to be carried out at appropriate intervals. There now seems considerable reason to hope that this may at length be undertaken. With the aid of grants made by the Eugenics Society and the Nuffield Foundation, the Scottish Council for Research in Education has initiated an inquiry of the kind proposed; and this, it is stated, is to be "the first of a series of such surveys". A preliminary account of the results so far obtained has now been published. The investigators are careful to point out that "no one associated with the proposal imagined that the survey would give a final answer to the question—is intelligence declining?" Nevertheless, several reviewers, particularly in the popular press, have presumed that this was the original intention, and that the 'final answer' is really the exact opposite of what had been expected. Others have contended that, if we admit that the results are inconclusive, this of itself must cast doubt on the value of the original proposal. Accordingly, I have thought it desirable to add a few brief comments dealing both with the methods of the survey and with its results.

I should like to thank first the numerous correspondents who have written about the subject of this paper, and secondly the new publishers, Messrs. Cassell & Company, both for suggesting that a fresh edition should be printed and for permitting me to make the additions I have described.

C. B.

January, 1952

PREFACE TO FIRST EDITION

THE following survey was originally prepared at the request of the Royal Commission on Population. The problem raised is briefly this. In this country, as in many others, the birthrate is known to differ widely in different families and in different sections of the community: is there any evidence that these differences are likely to alter the inherited mental qualities of the nation, and in particular to lower its level of innate general intelligence? Since the Commission asked primarily for an account of my own inquiries and conclusions, I have made no attempt to review the general literature of the subject or to describe in any detail the work of other investigators. From the short summaries that I have given, however, it will be seen that, on all the fundamental points, there appears to be, with one or two outstanding exceptions, a remarkably close agreement.

Much of my data was collected while I was serving as Psychologist to the London County Council; and a large part of what follows is taken from my official reports. The main figures have been printed in earlier publications; but the details of the calculations are not readily accessible. I am therefore greatly indebted to the Education Officer of the Council for permission to publish them here; I am asked, however, to state, in accordance with custom, that neither the Council nor its officials can accept responsibility for the opinions or conclusions reached. At the same time I should like to repeat my thanks to the numerous teachers, care committee workers, and research-students who assisted in the inquiries, whether in London or in other parts of the country.

To the Eugenics Society and to its Secretary, Dr. C. P. Blacker, I am deeply grateful for suggesting and arranging for the publication of these notes. We have also to acknowledge the courtesy of the Royal Commission for allowing their issue in this form.

C. B.

15th March 1946

INTELLIGENCE AND FERTILITY

THE EFFECT OF THE DIFFERENTIAL BIRTHRATE ON INBORN MENTAL CHARACTERISTICS

The Argument

ON the issues raised, the most reliable evidence so far available consists of data collected during surveys of school populations by means of tests of intelligence. By intelligence is meant inborn, general, intellectual ability. During the past forty years work with standardized tests has conclusively established (i) that a general factor underlying all forms of mental efficiency may plausibly be assumed, (ii) that this factor can be assessed with reasonable accuracy by both group and individual testing, (iii) that the differences in intelligence as thus defined and measured are due largely to the individual's inherited constitution.

So far as the evidence goes, (*a*) it seems almost certain that there is in this country a negative correlation between innate intelligence and size of family, and that the size of the correlation (about — 0·20) is large enough to demand urgent practical attention; (*b*) it seems highly probable that the average level of intelligence among the general population may be declining at a rate which might produce serious cumulative effects if at all sustained; (*c*) finally, it seems more probable than not that, with characteristics other than intelligence (e.g. temperamental or moral qualities such as relative freedom from neurotic or delinquent tendencies, and physical characteristics such as health and strength), the effects of the differential birthrate are smaller, but, if anything, unfavourable rather than favourable.

The evidence, however, is by no means conclusive. In psychology methods of measurement are subject to so many disturbing factors that the empirical verification or disproof of such inferences is a far more difficult matter than in the less complex sciences. Even as regards intelligence, there is as yet little direct confirmation irrefutably demonstrating an actual decline. And our theoretical knowledge of the inheritance of human mental characteristics is at present far too meagre for indirect deductions to be completely trusted.

The calculated figures suggest a drop of nearly 2·0 I.Q. points per generation (that is, expressed in terms of mental age, about a third of one year's mental development). On the other hand, a comparison of

the actual figures for recent years with figures obtained a generation ago reveals changes in the number of the bright or the dull children decidedly smaller than this rate of decline would imply. Thus, while both lines of evidence suggest the existence of a decline, they differ in the amount indicated.

The figures obtained in different parts of the country by different investigators show a striking agreement. Yet, for the reasons given, the only conclusions which, I believe, every psychologist would accept as beyond all reasonable doubt are the following: (1) there is an overwhelming case for a carefully planned inquiry on a large and systematic scale to test the inferences set out above; (2) the inquiries already carried out show that the psychological and statistical techniques are now sufficiently developed to make a full-scale inquiry well worth while, provided it is planned and carried out by investigators adequately trained.

(1)

Early Investigations on Selected Groups

The earliest systematic inquiries on the inheritance of mental characteristics in the general population were those undertaken by Karl Pearson and his fellow-workers, largely under the inspiration of Sir Francis Galton. These were of great value in demonstrating the need to substitute a scientific procedure for casual observation and subjective impression. But they were still largely vitiated, as has often been pointed out, by reliance on impressionistic ratings of popularly conceived intellectual and moral qualities. Galton, indeed, had suggested the possibility of inventing psychological tests; but his suggestions were mainly confined to tests requiring simple sensory discrimination or speed of reaction.[1]

1. At Oxford

The first attempt to attack the problem by means of formal tests of well-defined psychological characteristics was, I believe, a research [2] carried out at Oxford by J. C. Flugel and myself in 1907 at the suggestion of William McDougall.[3] Galton's proposal for an

[1] Cf. J. C. Flugel, *A Hundred Years of Psychology*, pp. 126–33.
[2] Burt, C., "Experimental Tests of General Intelligence", *Brit. J. of Psychol.*, III, 1909, pp. 94–178, and other papers.
[3] Cf. McDougall, W., "Psychology in the Service of Eugenics", *Eugenics Review*, VI, 1914. Cf. also *id.*, *National Welfare and National Decay* (Methuen, 1921).

anthropometric survey of the whole country, including mental as well as physical characteristics, was then very much to the fore. My own view was that, so far as intellectual characteristics were concerned, the easiest plan would be to begin with surveys of the child population in the schools. Accordingly, the primary aim of the Oxford experiment was to discover a set of psychological tests, sufficiently reliable and sufficiently practicable for inclusion in such surveys. The first set of tests were tests of cognitive (i.e. simple intellectual and practical) capacities; and ranged from the lowest and most elementary sensory and motor capacities up to more complex processes on higher mental levels, such as memory, attention, reasoning, and the like.

One of Karl Pearson's fellow-workers, David Heron,[1] had recently published considerable evidence demonstrating that the birthrate differed widely with differences of social level, that of the professional classes being less than half that of the so-called working classes. It was therefore arranged that two groups of children, representing these two contrasted social classes, should be examined. Complete age-groups (12·0–13·0 years) were chosen (i) from a well-known preparatory school (the Dragon School), where the pupils were sons of men eminent in the intellectual professions (bishops, professors, scientists, civil servants, etc.), and (ii) from an elementary school, where the parents were local tradespeople and working men, not so ill-paid, however, as to lead to serious handicaps from poverty or poor health.

Every child was tested individually; and each test was applied twice by two independent investigators (Flugel and myself: the coefficient of agreement averaged about 0·8). In our first inquiry, owing to the large number of tests included, only small groups could be examined—30 boys at the elementary school, and 13 at the preparatory. In spite of the small numbers, the essential differences were significant. It was argued that "the tests chosen do not involve to any appreciable degree acquired skill or knowledge: they consist of simple

[1] Heron, D., *The Relation of Fertility in Man to Social Status* (1906). In an analysis of birthrates for London boroughs Heron showed that the correlation of high birthrate with lower social class had almost doubled between 1851 and 1901. The relation emerged still more clearly in later analyses of the data obtained in the census of 1911 and of 1921: for the educational implications, cf. E. J. G. Bradford, "Can Present Scholastic Standards be Maintained?", *Forum of Education*, III, 1925, pp. 186 f. Pearson and his collaborators found no correlation between fertility (size of family) and intelligence (Pearson, K., and Moule, M., *Ann. Eug.*, I, 1925); their data, however, were obtained from a somewhat selected population, viz. pupils attending the Jews' Free School, Aldgate: the assessments for intelligence were merely ratings allotted according to the biometric laboratory's scale.

mental tasks unlike anything the children had ever previously prac-
tised. Hence there is reason to believe that the differences revealed
are mainly innate." With two minor exceptions (the two lower senses,
touch and weight) the average performances of the preparatory boys
were in all cases superior to those of the elementary boys, indeed, in
most cases superior even to those of the best elementary boys.

Moreover, the class-difference was greatest in those tests which
appeared to be most highly correlated with "intelligence". A detailed
analysis of the results led to the final conclusion that "at least one
mental character of the highest 'civic worth', namely, intelligence, can
be reliably measured and appears to be inherited". Supplementary
investigations, which included similar studies with mentally defective
children attending special schools, appeared to strengthen these
inferences, and "to support the growing apprehension that the
elimination of unfit stocks is being gradually suspended, and that the
dull and defective are multiplying more rapidly than the bright ".[1]

Conclusions. The following conclusions were drawn from these
early investigations : (i) As Galton, Binet, Spearman, and others had
maintained, and contrary to the view then held by Thorndike and
his followers, it is perfectly legitimate to speak of a "general ability"
entering into all intellectual functions. (ii) This "general ability"
can best be assessed, not by impressionistic ratings (Pearson and
others), nor by a test of simple sensory discrimination (Galton and
Spearman), but by a combination of tests involving higher and more
complex mental functions. (iii) "General ability" as thus assessed
appears to be largely inheritable and therefore innate. This led
to the definition of "intelligence" as "innate general intellectual
efficiency" (I now prefer to substitute "cognitive" for "intellectual",
since "intellectual" may suggest that "practical" efficiency is ex-
cluded). (iv) The average general ability is higher in children of the
professional classes than in children of the working classes, though
the differences between individuals appear far greater than the
differences between classes. (v) If intelligence is innate, then the
declining fertility of the more intelligent must raise a serious social
problem.

2. *At Liverpool*

In order to apply test-methods to larger numbers, Mr. R. C. Moore

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[1] The experiment was repeated at Oxford a few years later by another
of McDougall's pupils, with slightly different tests but with the same
essential result (cf. H. B. English, "Mental Capacity of School Children
correlated with Social Status", *Yale Psychological Studies*, 1917).

and I carried out experiments with "group tests": these are tests based on roneo'd or printed question-sheets, similar to the examination-papers commonly set by teachers and others in testing ordinary school knowledge: they differ (i) by depending as little as possible on differences in acquired knowledge or skill; (ii) by including 50 to 100 short questions instead of a small number of long questions; (iii) by requiring the children to answer with single words only, or simply to underline alternative answers; (iv) in later versions, by presenting the problems in non-verbal form. It was found that for higher mental processes, particularly general intelligence, and with older pupils, the group method of testing was nearly if not quite as reliable as individual testing. Further, unlike the tests involving special apparatus, they could be applied and marked by any teacher with experience in the ordi ue of school examination.[1] With the ily to the assessment of more extensive scale were o rural areas. In all these invariably appeared; and ering general intelligence, innate. At the same time, ered in innate general ents and certain special

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XXIII, 1926,
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In a supplementary inquiry, attempts were made to assess other intellectual, practical, emotional and moral qualities. The chief conclusions were: (i) that surveys of such qualities were of less importance theoretically and difficult to carry out in practice, and (ii) that emotional and moral qualities were far more amenable to environmental influences than intellectual capacity. It was therefore argued that "a community, which allowed low grades of intelligence to multiply and then tried to train them, and which neglected the training of emotional and moral qualities, was pursuing an extremely unwise policy".[1] The eugenic corollaries which apparently followed from these various results were discussed in a paper read before the Eugenics Society and in two or three semi-popular articles published in the *Eugenics Review* and elsewhere[2]; but at that date the entire hypothesis of a "general factor" of intelligence came under sharp criticism from Dr. William Brown and Prof. Godfrey Thomson in this country and from Prof. E. L. Thorndike in America.[3]

(2)

Later Investigations on Unselected Groups

From 1913 to 1932, as Psychologist to the London County Council, I was able to make far more extensive investigations bearing upon this problem, during the course of my regular work in the elementary and secondary schools of the County. Among the official duties of the psychologist, the following were given first place: (i) to devise and apply tests of innate ability for selecting mentally deficient children for special schools—tests, that is, which should not be affected by home

[1] It may be remembered that at this date the aim of the special m.d. school was to improve the feebleminded children, so that they could be decertified; and little or no systematic effort was made at treating juvenile delinquents or (outside the public schools and a few older grammar schools) at any kind of character-training.

[2] E.g. Burt, C., "The Inheritance of Mental Characteristics", *Eugenics Review*, IV (1912), pp. 1–33; "The Experimental Study of Intelligence", *Child Study*, IV (1911), pp. 14–44; V (1912), pp. 1–33. Cf. also Edgar Schuster, *Eugenics* (Collins, 1912), pp. 153, 219 *et seq.*

[3] Cf. Brown, W., and Thomson, G., *Essentials of Mental Measurement*, Chap. IX, Sect. 3 and refs. In view of the more favourable opinions subsequently expressed by all three critics, we may, I think, take it that their criticisms were directed, not so much against the theory itself, as against the weakness of the evidence then available.

or school handicaps, and should thus avoid the injustice often done when such cases were selected solely by the impressionistic judgments of teachers or school doctors; (ii) to devise and apply tests of innate ability for selecting scholarship children—tests once again which, unlike the ordinary examinations in English and arithmetic, should not be influenced by the superior home or school facilities of better-off families; (iii) to determine from time to time whether the average level of educational attainment throughout the Council's schools was improving or declining owing to change in teaching-methods or other causes.[1]

In pursuance of these aims, part of the general plan of work was to carry out, at convenient intervals and with the aid of teachers, care committee workers, and others, (i) broad surveys of London schools as a whole, (ii) more detailed surveys of the schools in a single representative borough, and (iii) still more specialized studies of particular groups—the mentally defective, the dull and backward, the supernormal, the neurotic, and the delinquent—side by side with control-groups of normal pupils. In the most thorough of these surveys an endeavour was made to obtain as complete a picture as possible for the school population of a typical borough: the device of "representative sampling" was adopted; and 3,510 children were tested with an English adaptation of the Binet-Simon scale. Later surveys were carried out in the same area with a revision of the Terman version; and, since the majority of the tests in the two versions were identical, an exact comparison could be made. The test-results were always submitted to the children's teachers for comment; and, so far as possible, doubtful cases were checked by perceptual tests (usually the Porteus Mazes, the London version of the Pintner-Paterson performance tests, or non-verbal forms of the analogy, matrix, completion, and classification tests).

Distribution of Intelligence among the General Population

The earliest of these surveys showed conclusively that variability in intellectual and educational characteristics "appears to be of much the same order as physical variability"[2]; and this was amply confirmed by later inquiries. Like the distribution of stature, the distribution both of educational attainments and of innate intelligence was found to conform, at least to a first approximation, with the so-called normal curve. And like the standard deviation for stature and weight, the standard deviation for intelligence proved to be, at the middle of

[1] Cf. L.C.C., *Annual Report of the Psychologist*, 1915.
[2] L.C.C. Report No. 1868 (1917), p. 86.

the school career (age 10), roughly equivalent to about $1\frac{1}{2}$ years' growth. On the other hand, the standard deviation for attainments was equivalent to only 1 year's progress at the age of 10. Thus education tends to diminish, not (as is sometimes argued) to increase, the variability revealed by intelligence tests.[1]

Three features of these distributions deserve emphasis: (i) The range of intelligence appeared far wider than had previously been supposed: thus, with a typical sample of 100, the mental ages of elementary school children aged 10 by the calendar range from about 6 years up to nearly 14 years, and show a standard deviation of about 15 points on the I.Q. scale. (ii) Between the lowest type of mental defective and the highest type of intellectual genius the gradations are continuous: there is apparently no gap or gulf; in a sufficiently large sample every intervening grade of intelligence will be represented. (iii) If we may trust the assessments of inborn ability, the average level of the adult population appears to be not much higher than that of an average child of 14.

It is important to note that the form (especially the symmetry), as

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[1] L.C.C. Report No. 2052 on *Mental and Scholastic Tests*, 1921, p. 176, and Fig. 24 facing p. 162 (based on 3,600 children). Cf. also L.C.C. Report No. 1868, on *The Distribution and Relations of Educational Abilities*, 1917, Fig. 6 facing p. 33. The sizes of the school samples were large enough to show that the distribution departed significantly, though not considerably, from perfect normality. In fact, the curve found was rather like that which would be expected if we supposed that the distribution of intelligence is determined, not by an indefinitely large number of indefinitely small causes, but by a large but definite number of dominant and recessive genes. However, much of the amount and direction of the skewness is, in my view, likely to be an effect mainly of the mode of measurement: if, as in the early Binet scale, the upper end of the scale is limited, the skewness is likely to be negative; if it is unlimited, the skewness is likely to be positive, (a) because a child of 8 cannot deviate more than 8 years *below* the average, whereas he may quite possibly deviate more than 8 years *above* the average, and (b) because the intervals between age-norms above 12 or 13 are apt to be compressed, owing to the retardation of mental growth.

Most psychologists have accepted the view that intelligence is distributed normally. E.g. Terman about the same time published data indicating that the distribution of intelligence quotients obtained with his version of the Binet Tests in America approximated to the normal curve, though no statistical test of normality was actually applied (*The Intelligence of School Children*, 1921, pp. 34, 50, 78). He suggested that the standard deviation with his revision was about 13 points, but later seems to accept 15 points (*Genetics of Genius*, III, p. 29). A fuller examination of the normality of the distribution is to be found in Thorndike, *The Measurement of Intelligence*, 1928, Chap. VIII: he reaches much the same conclusions, "a very little positive skewness, increasing year by year; and a slight swelling of the lower tail, due to pathological cases of mental deficiency".

well as the standard deviation, of the distributions may be appreciably affected (*a*) by exclusion or inclusion of children not attending ordinary elementary schools, and (*b*) by the particular test-scale employed, especially by the unit of measurement. A larger standard deviation is furnished by newer test-scales, as contrasted with the old, and by group tests as contrasted with individual tests.

To obtain a satisfactory picture of the distribution of intelligence among adults is not so easy. After the war of 1914–18, I was asked by the Civil Service Commission to carry out tests of intelligence on ex-Service candidates; and I then found that the distribution of intelligence among over 3,000 adults agreed, both in form and in general range, with that already observed among school children.[1] More recently, as a member of the War Office Advisory Committee on personnel selection, I have had occasion to analyse much of the material collected during the recent war from psychological tests used in the fighting services. Here, too, much the same conclusions appear to emerge. However, for publishable evidence from this source it will be necessary to await the release of official reports on the statistical results.

Distribution of Intelligence according to Occupational Class

From time to time an endeavour was made to assess the average intelligence of children and adults classified according to occupations. The table [2] on page 14 gives the most recent results as being the most reliable.

The correlation between children's intelligence and economic status amounts to 0·32. Other investigators,[3] both in this country and in America, have reached a figure of much the same order. The only

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[1] *Ann. Rep. Brit. Ass. Adv. Sci.* (1923), Section J (Psychology, Pres. Add.), pp. 215 f.

[2] Taken from "Ability and Income", *Brit. J. Educ. Psych.*, Vol. XIII, 1943, p. 84. The data for children refer to pupils attending London schools only. The data for adults were largely obtained during investigations carried out for the National Institute of Industrial Psychology: cf. Burt, *et al.*, "A Study in Vocational Guidance" (Medical Research Council Report No. 33), H.M. Stationery Office, 1926.

[3] For a brief summary cf. *Brit. J. Educ. Psych., loc. cit.*, p. 84. It has been alleged that, since the psychologist is himself a member of the professional class, he will naturally wish to demonstrate by his tests that his own class is superior in intelligence, and hence that the very tests he invents will be biased by his own class-notion of what intelligence is. With regard to the interpretations of intelligence, I may refer to the appendix, where it will be seen that the psychologist understands by this word a factor defined

TABLE I

Intelligence of Parents and Children classified according to Occupations

		Average Intelligence Quotient	
	Occupational Category	Children	Adults
Class I .	Higher professional: Administrative	120·3	153·2
Class II .	Lower professional: Technical executive	114·6	132·4
Class III .	Highly skilled: Clerical	109·7	117·1
Class IV .	Skilled	104·5	108·6
Class V .	Semi-skilled	98·2	97·5
Class VI .	Unskilled	92·0	86·8
Class VII .	Casual	89·1	81·6
Class VIII .	Institutional	67·2	57·3

exceptions of importance are the correlations reported by Raymond Cattell, who gives figures of 0·89 and 0·92.

A correlation of only 0·32 implies a large amount of overlapping between the various social classes. As was stated in my Report, "the average intelligence of different social groups certainly differs, but the difference between the averages is far smaller than the difference between the individuals". The group-differences shown in the table may therefore be a little misleading unless we bear in mind what has already been said about the wide range of individual ability. Children from the "Higher professional class" comprise individuals from every one of the six intelligence-grades set out below (Table III), including the feeble-minded. Similarly, children from the "Unskilled" and "Casual classes" include central school pupils and even scholarship

technically and objectively by the results of his work. In reply to the alleged unfairness in the academic or verbal bias of the tests, it may be emphasized that precisely the same conclusions are reached with non-verbal tests of a practical, mechanical, or performance type. So far from favouring children from the professional or better-off classes, the inclusion of a psychologist among the examiners for junior county scholarships almost invariably increases the proportion of scholarship winners coming from the poorer or the working classes. May I quote from a book by one of the most eminent of educational psychologists? "When I was 13 years of age and finishing an elementary school education, I won a scholarship to a secondary school. . . . I have ever since been greatly impressed by the influence that event had on my life; and have spent a great deal of time in endeavouring to improve the methods of selecting pupils at that stage." The vast majority of the psychologists now working in the educational field could, I fancy, echo that statement.

winners: indeed, owing to their greater numbers the humbler social classes produce far more pupils of genuine scholarship ability than do either or both of the professional classes.

During the last 30 years, the correlation between economic status and children's intelligence has, if anything, slightly increased. It would seem that the effect of educational and vocational guidance has been rather to enhance the tendency to a class-segregation according to intelligence. An exceptionally bright boy of working-class parents may now be fairly sure of a scholarship to a secondary school and a University; and, with or without the assistance of a vocational psychologist, he is likely to be in effect removed from the working class and transferred to the professional class.[1]

(3)

Relation between Intelligence and Size of Family

In the borough chosen as fairly typical of conditions in London as a whole, and made the subject of a more intensive survey, details were regularly obtained in regard to size of family (as well as other items). For these we relied mainly on the statements of the children themselves: but, where it seemed desirable, the data were checked so far as possible by the teachers or the organizer of children's care. At the poorest school in the borough, the average number of children in the family was 5·2; at the best school only 2·9. The average intelligence quotients were 98 and 113 respectively. Thus at ten, children from the poorest social classes not only have an intelligence that is nearly two years below that of the children from the better classes, but are also drawn from families nearly twice as large.[2] Similar tendencies appear if we contrast the exceptionally bright with the

[1] Since the last war, there have been some slight signs of a reaction against these ambitions. The words of one working-class father indicate the views of a good many: "I would rather see my boy stop on at the elementary school and be top of his class than go to the secondary school and drop to the middle of his form; if he has brains, I would rather see him leader among those of his own set than a nobody among a group of bank clerks or teachers."

[2] Cf. L.C.C. Report No. 2052 (1921), pp. 190–1. I may add that very much the same facts were noted in a survey which I was asked to carry out for the Birmingham Education Authority (*Report of an Investigation on Backward Children*, City of Birmingham Stationery Department, 1920, pp. 15 f.) and in an informal study in several Warwickshire villages.

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exceptionally dull: for example, as stated in my L.C.C. report, "the average number of children in families containing at least one backward child is 4.6; in families containing at least one scholarship winner it is 3.2; among scholarship winners, in 17 per cent. of the cases the pupil was an only child; but among the dull and backward, no more than 8 per cent. For the ordinary elementary school population taken as a whole, "the correlation between the intelligence of a child and the size of the family to which he belongs is − 0.19: the correlation between educational attainments and size of family is − 0.22". If, in addition, a due proportion of children from special m.d. schools and from secondary and other schools of a higher type are included, the correlation between intelligence and size of family rises to − 0.24.

But in considering surveys carried out by testing children at school we have to bear in mind two limitations. First, many of the families must be still incomplete at the time of the survey. Moreover, on an average, the parents of the brighter children are somewhat older; hence their families are more likely to be complete, while the families that include the duller children may still go on increasing. Secondly, unmarried and childless adults are of necessity unrepresented; and these include a disproportionate number of persons whose intelligence is above the average level. If due allowance be made for both these limitations, then the final figure for the correlation would be higher still. It may be added that, although the frequency distribution for family-size is asymmetrical, the regressions (except where the lowest defectives are concerned) appear to be remarkably steady and linear: (they imply an increase of nearly $\frac{1}{2}$ a child for a decrease of 1 S.D. in the intelligence scale, or about 1.4 I.Q. decrease for the increase of 1 child in the family). Thus the restriction and the non-restriction of births are not phenomena peculiar to certain social classes or certain intellectual levels.

Several social writers have argued that, since (as I pointed out) "the difference between the average intelligence of the social classes is far smaller than the difference between individuals within the same social group", it is quite possible that the inverse correlation between size of family and intelligence may after all be due simply to differences of reproductivity between the different social classes, and not associated with differences of intelligence as such (indeed, within the higher professional class some writers have actually reported a positive correlation between reproductivity and intelligence). But any generalization of this kind seems fully refuted by two lines of evidence. First, we can calculate the correlations obtaining within schools where the pupils are all recruited from a fairly homogeneous social and occupational class. We find that the correlations are still

significant, ranging from — 0·11 to — 0·18. This is so even in the poorer areas. The conclusion to be stressed, therefore, is not so much that the smaller, well-to-do or professional classes are producing few children, but that among the far more numerous working classes it is still the most intelligent families who contribute fewest to the next generation. Secondly, we may verify this inference on a wider scale by the device of "partial correlation". Taking a conservative estimate for the correlation between family-size and intelligence, viz. — 0·19, and an average estimate for the correlation between family-size and occupational class, viz. — 0·21, then, within a group of the same occupational level, the partial correlation between family-size and intelligence is still — 0·14—a fully significant figure and probably an underestimate.

At present, no doubt, social class, which so largely determines the aims of the parent for his child, may be a factor almost as important as intelligence—except perhaps in the highest intellectual group of all: it is when the bright child from the poorer classes has won his way to a higher social stratum, and has himself turned into a parent, that the desire to limit family-size becomes so marked; he himself perhaps was one of five or six children, but he produces only two or three. Nevertheless, with the rise in the standard of living and pleasure throughout the whole community, the class correlation will, I feel sure, grow smaller; and the partial correlation with intelligence will soon outweigh the partial correlation with occupational category or economic group. It has been perhaps a little unfortunate that the correlation with class, being the easiest to determine, came so early into the picture, and thus side-tracked the argument. Social origin and economic level, as such, may soon prove wholly irrelevant to the argument.

(4)

Geographical Distribution of Intelligence and Family Size

The correlations just cited were obtained in the course of a detailed survey of one large, typical borough. When we turn to London as a whole, a somewhat different line of approach suggests itself. Instead of procuring figures giving the intelligence and the size of family for individual pupils, we may obtain them for the constituent districts. In several of my earlier Reports, maps were drawn up showing the incidence of backwardness, mental deficiency, and

scholarship ability, and related social conditions (such as birthrate, size of family, and average economic level) for each of the electoral areas into which the county is divided.[1] These reveal, in graphic form, much the same correspondences as we have already noted for one particular district. Calculated by boroughs, the correlation between average size of family and percentage of backward and mentally defective pupils was found to be 0·35, and the correlation between average size of family and percentage of junior county scholarships — 0·21. By themselves, of course, such figures could not prove that the sole or even the chief cause of the variations were differences in innate mental characteristics; but they do imply that the tendencies observed in the borough which was more intensively surveyed also hold good throughout the county as a whole.

(5)

Estimating the Amount of Apparent Decline

Nevertheless, as was emphasized in my report, "some caution must be observed in drawing deductions from all these figures". Certainly, with the correlations in front of us, it might seem "almost impossible to avoid the conclusion that, owing to differential fertility, the average level of intelligence throughout the whole community has already begun to decline". Yet, when we seek to estimate the amount or rate of that decline, we are met by unexpected difficulties. All the methods of estimation agree in suggesting *some* degree of decline; but they differ appreciably in the amount indicated. In the absence of systematic inquiries on a precise and extensive scale, our only course must be the rather tedious process of comparing all the main lines of evidence, and noting the upper and the lower limits to which their joint results appear to point.

There are two principal ways in which we may attempt to assess for a given community the rate at which its average inheritable intelli-

[1] *Psychologist's Report on Junior County Scholarship Examinations* (1924). The map giving the distribution of backwardness was reprinted in *The Backward Child*, p. 97, and the corresponding table of educational and social statistics (with correlations) on pp. 100–1. As stated in the footnote (p. 99), the local distribution of the figures changed comparatively little between 1913 and 1932, but the correlations tended very slightly to diminish. Cf. also the map and table published in *The Young Delinquent*, pp. 73 and 77.

gence is changing—an indirect and a direct. (1) Given the average I.Q. for families of different sizes (or the differential fertilities for families of different I.Q.) we may attempt to predict the I.Q. level of one generation as compared with that of another. (2) Assuming the composition of the community to have been stable over an interval of time (or making due allowance for changes in apparent composition) we may measure the average I.Q. level at the beginning and the end of the interval by two separate surveys. In practice, both methods of approach are attended with numerous difficulties that may introduce considerable error. I shall consider first the results of investigations carried out in London, and then, more briefly, those obtained in other parts of the country.

The following estimates are based on figures secured by both procedures from L.C.C. schools (special, ordinary elementary, and secondary) for the same borough as before. Since the I.Q. varies considerably according to the type of test used, I have adopted the practice of expressing all figures in terms of a standard scale: the best scale for this purpose would seem to be the London Revision of the Stanford-Binet tests.[1]

(1) *Indirect Estimation.* (a) With only a single generation available for testing, the natural procedure is to count up the number of families having 1, 2, 3, . . . children, and determine the average intelligence for each type of family. We can then calculate (i) what would be the average mental age (or intelligence quotient), if every family were of the same size, (ii) what is the actual average mental age (or intelligence quotient) with each family weighted according to its size. By this method we estimated that, for the borough in question, and during a single generation, there would be a drop of 2 mental months at the age of 10·0–11·0, or of about 1·8 points on the I.Q. scale (the figure is raised to about 1·9 points if an allowance is made for married and unmarried adults of parental age having no children).

[1] This is the scale most frequently used by educational psychologists and child guidance clinics in this country. It was also adopted in the surveys carried out by the Joint Committee on Mental Deficiency (a description will be found appended to the Committee's Report, 1929, Pt. II, pp. 218–20). Owing to the limitations of the Binet scale towards the upper end, the higher mental ages are artificial norms based on group-tests, the size of the yearly intervals being determined by extrapolation. With this extended scale the standard deviation for the complete London population would appear to be rather over 15 I.Q.—about 16 for the upper half and rather less for the lower. Within the L.C.C. schools only, the standard deviations are reduced by the omission of ineducable imbeciles and idiots at one end, and at the other end bright fee-paying pupils attending private or public schools. These omissions reduce the standard deviation to 14·5 I.Q.—14·8 for the upper half and 14·2 for the lower.

However, when working in the schools instead of making a house-to-house survey, the figures obtained for the size of each family, and the frequency of each size, particularly in the case of the largest families of all, are apt to be somewhat untrustworthy. Hence it seemed desirable to check this estimate by an alternative method, working with average figures for family size. Though less accurate in theory, this procedure is probably more accurate in practice.

(b) For this complementary approach we require to estimate both (i) the proportionate number of children, and (ii) the proportionate number of families, having a mental age or I.Q. of a given level. Compressing the results into a table with wide intervals, we obtain the following figures:

TABLE II

Distribution of School Population over the I.Q. Scale

Approximate Type	I.Q.	Proportionate No. of Children	Average No. per Family	Proportionate No. of Families
1. Scholarship . . .	> 130	1·8	2·3	2·7
2. Central school . .	115–130	12·2	2·7	15·3
3. Good average . .	100–115	35·1	3·3	36·0
4. Poor average . .	85–100	37·5	3·6	35·3
5. Dull and backward .	70–85	11·9	4·2	9·6
6. Feebleminded . .	< 70	1·5	4·7	1·1
Total		100·0		100·0

The table may be interpreted as follows. Out of (say) 1,000 ten-year-olds the number having an I.Q. between 115 and 130 is 122; the number having an I.Q. between 70 and 85 is almost exactly the same, viz. 119. But in the former group the average size of the family is only 2·7, whereas in the latter it is 4·2. Therefore the latter must have been drawn from a far smaller number of families and so from a far smaller number of parents. In fact, with certain simplifying assumptions, we can infer that the actual number of families must be proportionate to 122 ÷ 2·7 and 119 ÷ 4·2, that is, to 45·2 and 28·3 respectively. Adding the figures obtained in this way, we reach a total of 295·1 families (this may be roughly thought of as the number of married couples in the previous generation required to produce the 1,000 children in the filial generation with which we are now concerned—though that interpretation cannot be pressed too strictly). Accordingly, to translate the proportions into percentages we must divide each figure by this total and then multiply by 100. Thus the

proportionate number of families in the two I.Q. groups mentioned must be

$$45 \cdot 2 \times \frac{100 \cdot 0}{295 \cdot 1} = 15 \cdot 3 \text{ per cent.}$$

and

$$28 \cdot 3 \times \frac{100 \cdot 0}{295 \cdot 1} = 9 \cdot 6 \text{ per cent.}$$

Similarly for the rest of the figures in the table.[1]

Now, if we calculate the average I.Q. for the whole group of children from the first column of percentages, we shall find that the general level of intelligence is 100·0 I.Q.: this figure follows of necessity if the entire group was a fair sample of the population on whom the tests had actually been standardized. But now let us suppose that the families in the area, instead of varying in size from 2·3 to 4·7 according to intellectual level, had each averaged precisely the same number in every one of the six groups. Then the average I.Q. (calculated now from the last column of percentages instead of from the first) would have been 101·9 instead of 100·0. Since the latter is the figure actually found, we are plainly faced with a drop or loss of 1·9 points on the I.Q. scale.[2]

[1] Actually the absolute numbers of children and of families were obtained first and the percentages and averages derived from these.

[2] Most investigators have sought to deduce the average intelligence of the previous generation from the data obtained from the child-generation that is actually tested. That means that the intelligence of each parent (or mid-parent) must first be estimated from the data in the sample. There are (as implied above) two ways of doing this. We may either (a) calculate the average I.Q. of each family, or (b) calculate the average size of each family grouped according to the I.Q. level of the children: the former seems the more appropriate. A critic, however, will object that such deductions must surely be somewhat precarious, since, after all, we have tested no members of the parent-generation. For instance, in assuming (as some investigators have done) that the intelligence of the mid-parent in a given group is identical with that of the children in that group we ignore the fact of regression. To my mind the simplest reply is this. What we are here primarily calculating is the *loss* of I.Q. resulting from unequal fertility; what we are contrasting is the actual level and the possible level of the same generation, not the actual levels of two different generations.

Moreover, as noted in my original Report, there is a further reason why it may be rather rash to deduce the relative intelligence of the parents from that of the children tested. There would seem to be a negative correlation between intelligence and order of birth. With my own cases it amounts to only −0·12; it appears to vary considerably in different social classes, and to have diminished during the last 25 years. Nor is it altogether simple or linear: on the average, the brightest children are apparently, not the first-born, but the second; the third and fourth have slightly lower I.Q.'s, after which the decline is more marked: the best-established instances are those

Once again, allowance should be made for the facts that many adults have remained childless, and that many of the families, particularly in the lower grades, were doubtless not complete at the time of the survey. Such allowances would increase the estimate by several decimal points. On the other hand, in spite of the care taken in constructing the tests and in re-examining doubtful cases, it would certainly be unfair to attribute the whole of the difference thus revealed to differences in innate or hereditary constitution (see Appendix I); and a due allowance for this defect would probably knock off the decimal points we have just added. In fact, after giving full weight to both these opposing factors, we should, I think, eventually conclude that, when based on the foregoing figures, the most plausible estimate would still be in the neighbourhood of about 1·9 I.Q.

(2) *Direct Estimation.* Now experience shows that, where many of the factors are obscure or unknown, it is always wise to secure direct confirmation, so far as possible, for any conclusion indirectly deduced. And an educational psychologist, familiar with the watchfulness of teachers, would expect that, if a decline of this size had been going on for at least a generation, many of the older head-teachers and officials would assuredly have noticed some of the incidental effects—a decrease in scholarship ability or an increase in the dull and mentally defective. Further, in those areas where the same schools have been re-tested after an appreciable interval, there should be discernible indications of the change in the test-results. We might expect this more direct method to yield a larger figure than the more indirect; for the results of the later survey would include the effects of the childlessness of so many superior adults. At the same time, we must remember that the probable error of the assessments is bound to be high: for example, even if complete samples of 4,000 children could be tested on two separate occasions, the probable error of the difference between the two means could hardly be less than 0·25 I.Q.

Between 1913 and 1939 I endeavoured to assess the average level and variability of the London school population on three occasions—namely, when standardizing, first the original Binet scale, secondly

of Mongolian defectives, who often appear at the tail of a rather large family. Since we are testing fairly young children, it would follow that, whenever we are dealing with members of a large family, we must usually be testing those who are late in order of birth; and (if the foregoing evidence is accepted) these will be the duller representatives of the family. However, so far as my data go, the differences associated with order of birth are decidedly small—only a fraction of an I.Q. Hence it may be doubted whether any serious errors have arisen from the fact that this point has been so often overlooked.

the Stanford revision, and thirdly the more recent version by Terman and Merrill. The third inquiry was unfortunately cut short by the war, but has now been resumed. The best of the Binet tests are retained in all three versions. Hence by basing the I.Q.'s on these tests alone we can obtain comparable figures. These have been further checked by repeating the same group tests.

(a) On taking the average I.Q. for fairly large and representative samples, and averaging the differences obtained from comparable groups, I find an average rate of decline amounting to 0·87 point in 20 years, or about 1·3 points in one generation. None of the reliable differences exceeds 1 point in 20 years, or about 1·5 points in a generation.

(b) Slightly more extensive data are available for estimating the proportions of pupils who at the ages of 10–11 fall outside a borderline of 70 I.Q. and 125 I.Q. respectively. These borderlines fall within the zone of doubtful cases for certifiable deficiency and for junior county scholarships respectively—cases for which individual examinations and tests are fairly frequent. So far as the estimates can be trusted, it would seem that within the space of 20 years (1912–32) the number over an I.Q. of 125 dropped from 5·93 per cent. to 5·12 per cent.; while the number below an I.Q. of 70 rose from 1·54 per cent. to 1·71 per cent. If we ignore the possibility of extraneous influences, a drop of the former type might be expected from a decline in the mean I.Q. from 100·0 to 98·9; a rise of the latter type from a decline from 100·0 to 99·5; i.e. in 30 years a decline of about 1·65 and 1·00 points respectively.

A wide margin of error must be allowed for imperfections in sampling, testing, and smoothing gradients. And even if we ignore these sources of error, it would still be somewhat rash to accept such estimated changes as due solely to differential fertility. After an interval of 20 years the families residing within a given London borough are no longer the same. In all except the poorest boroughs about 30 per cent. of the population have been born outside the county. On the other hand, many of the families, particularly among the better classes, have moved beyond the boundary. While in some areas slums have been cleared, in others good houses have deteriorated, and have been let as apartments for poorer newcomers. On striking a balance, it would seem that, on the whole, such migrations must themselves have led to a slight decline. This seems confirmed when we restrict the comparisons to families who have remained in the borough most carefully studied throughout the whole period: however, this mode of comparison involves dropping rather more families of a better type, so that the figures must still be somewhat

affected by selection of the same type. On attempting to make due allowances I reach a figure between 1·0 and 1·5.

It will be noted that, particularly when we attempt to allow for these irrelevant factors, there is an appreciable discrepancy between the estimates reached with indirect and direct methods respectively. The indirect method suggests a decline of about 1·9 points, possibly more; the direct method suggests a decline of 1·5 points at the outside, probably much less. Of the two figures, the higher seems the more questionable. Were we to accept at their face value the proportions reached by the indirect method and set out in Table I, we should infer that the rate of change in the proportions of bright and defective children had been decidedly rapid. Taking the proportionate number of parents of a "scholarship type"[1] or a "mentally defective type" respectively as given in that table, we should conclude that, in the preceding interval of about 30 years, the number of the former had dropped from 2·7 to 1·8 per cent., and the number of the latter had risen from 1·1 per cent. to 1·5 per cent. I find it very difficult to believe that changes so large and so rapid as this have actually been taking place. They would imply a decrease or an increase of nearly 50 per cent. in the number of children awarded junior county scholarships or certified as mentally deficient. Changes so great as this, even if spread over 30 years, could hardly fail to excite spontaneous comment from teachers, medical officers, and officials of the Education Department.

A critic may perhaps object that, since all London children are subjected to much the same type of elementary education, its effects may easily obscure the actual amount of decline when we try to assess it by mental tests. But the objection seems sufficiently answered by the fact that changes resulting from migration are not obscured. They remain plainly discernible in the test-results: e.g. in certain smaller

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[1] The designation "scholarship type" was originally based on the award of junior county scholarships in London at the beginning of my work. In 1912–13, out of 82,000 children aged 11, 1,655 children obtained such scholarships—a proportion of just over 2·0 per cent. This suggests a borderline of + 2·05 S.D.; that is, with an S.D. of about 15 I.Q., a borderline of 130 I.Q. After that date the proportion rapidly increased; and today the phrase "junior county scholarship type" would be almost devoid of meaning. However, the borderline roughly coincides with the lower level of those who secure entrance scholarships to Universities and obtain an honours degree. Consequently, the term may perhaps be permitted to stand as a convenient label for the top category. By "mentally defective" is meant children certified as fit for "special schools for the mentally defective". Here the borderlines are 70 to 50 I.Q. The lower figure excludes ineducable imbeciles and idiots, who would bring the proportion to just under 2·0 per cent.

districts, from which slum populations have been removed, and in other districts into which such a slum population has been transferred, a comparison of the test-results obtained before and after the transferences (in several cases, 5 to 8 years afterwards) has revealed a well-marked shift in average mental level—sometimes as much as 6 points. Indeed, not only in London, but in industrial and rural areas outside the county, the greatest and most readily established changes in the average I.Q. seem traceable to migration rather than to alterations in the native endowment of the stationary elements of the population.

However, the possible effects of environmental influences may be considered from another point of view. During the last 20 years teaching methods for dull and backward pupils have greatly improved; better health facilities, both before and during school years, must have appreciably increased their physical fitness. Consequently, those who hold that such conditions must have a discernible influence on test-results might argue with some plausibility that the apparent amount of decline must form an underestimate. Personally I should attach little importance to this argument. An experienced psychologist, diagnosing mental deficiency in borderline cases, never accepts the results of a given test, just as they stand, without considering the possible effects of educational or physical handicaps. Hence, at any rate in the figures for mental defectives, no further allowance seems necessary.

But there is yet a third possibility, which perhaps deserves consideration. It is tempting to speculate whether the commoner methods of forecasting changes in mental level indirectly from the I.Q.s of families of different sizes may not assume an over-simplified mechanism for human heredity. It is conceivable, for example, that some kind of regressional or compensatory process may be at work, which is not allowed for by the usual methods of calculation, and which tends to keep the mean of the population (as well as the variability of the population) rather more constant than the usual methods of computation would imply. Statisticians have repeatedly drawn attention to the fact that certain statistical characteristics of human populations show an unexpected constancy—as if, once a biological equilibrium has been reached, the population tends to preserve it, in spite of disturbing changes.[1] Most of the evidence adduced for this notion is

[1] The most familiar instance is the widespread belief that, when a tribe or nation loses a large proportion of its males in war, Nature at once redresses the balance by temporarily increasing the ratio of male to female births. The theory of statistical stability has its best-known advocates in Quetelet (*Corresp. Math. et Phys.*, VII, 1832, pp. 321–46) and Lexis ("Uber die Theorie der Stabilität Statistischer Reihen", *Jahrbuch f. Nat.*

admittedly inadequate; yet it is enough to make the theorist pause before prophesying an inevitable change on the basis of apparent trends. In genetics the classical experiments on "pure lines" remind us that to predict an alteration in traits from a knowledge simply of the selective factors apparently at work may be altogether rash in the absence of any supplementary knowledge about the way those traits are genetically determined. On the other hand, the dysgenic effects that might be anticipated from the selective incidence of the deathrate during wars have been more often falsified than confirmed by subsequent attempts to verify the predicted changes (compare, for example, Gini's examination of the effects of the last war on the population of Italy).

For all these reasons I am convinced that far more extensive studies are required before we can venture to specify a definite figure for the actual amount of decline in any given area. If required to make a guess at the most probable figure for London, I should be disposed to put it as nearer 1·5 I.Q. points per generation than 3·0 points.[1] I should be surprised if it was much lower than 1·0 point or much higher than 2·0 points.

(6)

Rate of Decline in the Country as a Whole

From investigations made elsewhere, either personally or with the aid of research-students, I am inclined to conclude that, in other industrial towns, the rate of change cannot differ greatly from that obtaining in London, but that in rural areas it is, in all probability, appreciably larger.[2] On an average, I suggest 1·5 points (or slightly less) for urban areas and 2·0 points for rural areas.

The estimates which appear to diverge most from my own estimates are those subsequently obtained by one of my former

Okon. u. Statist., XXXII, 1879, pp. 604–23). Weak as they now seem, some of their arguments at least merit reinvestigation. It is unfortunate that nearly all psychological discussions on the effects of differential fertility are still couched in terms of pre-Mendelian notions of heredity. For the views of the modern geneticist on the problem, I may venture to refer to Prof. J. B. S. Haldane's book on *Heredity and Politics* (1938); he suggests practically the same figures as I have reached in my own reports ("1 or 2 points per generation in the mean I.Q. of the country", p. 117).

[1] This higher figure is Cattell's: see below.

[2] As mentioned elsewhere, the other areas which I myself investigated were Liverpool, Birmingham, and certain Warwickshire villages.

students, Dr. Raymond Cattell, in Leicester and Devonshire.[1] His double survey is one of the largest hitherto made; and covers 3,734 school children. His figures indicate a decline of 3·1 I.Q. in the urban area and 3·3 I.Q. in the rural area. He finds that, during one generation, there has been "approximately a 30 per cent. increase of mental deficiency" and that (at any rate in the country) "the very able children have been cut down by almost a half". He explains the divergent estimates by saying that "London is scarcely typical of urban life". Even so, however, I feel sure that such a "galloping plunge towards intellectual bankruptcy" (as he describes it) would have aroused spontaneous comment from the school teachers and education officials. In my own view the divergence is more probably due, not to difference of locality, but to differences (i) in tests and (ii) in sampling.

(i) As my own figures had been criticized on the ground that the tests used might be considerably affected by educational and social facilities, he decided to use a non-verbal group test. As he explains in his preface, we discussed together the general methods to be adopted in his test-survey; so that, in the main, the procedures used should be comparable. Now, as his diagrams show, his test yields an S.D. of 19·4 I.Q. for the lower half of the distribution and 24·2 I.Q. for the upper half. Thus 3 points on his scale would correspond with only 2 points on mine. In spite of this change in the S.D., he retains my borderline of 70 I.Q. for mental defectives. As a result, 5 per cent. of the Leicester elementary school children are classed mentally defective (4·2 per cent. of the sample).

(ii) In his Leicester sample over 20 per cent. of the children were drawn from preparatory, public, or private schools. An inclusion of so large a proportion with an exceptionally low birthrate would of itself tend to exaggerate the amount of the apparent decline. If due allowance were made for these features, Cattell's figures would approach much nearer to my own.

All other investigators report figures suggestive of a comparatively small rate of decline. In Sheffield, Bradford [2] tested 393 boys and girls with the Otis test, and found a correlation of − 0·25 between intelligence and size of family: the average size of family decreased from 4·4 to 2·6, with an increase in test-score from 5 to 45. With the London revision of the Binet scale, this would (I estimate) amount to a decline of about 1·9 I.Q.

Some of the most instructive studies in this field are those initiated by Prof. Godfrey Thomson. I venture to think his conclusions all the

[1] *The Fight for Our National Intelligence*, P. S. King, 1937.
[2] Bradford, E. J. G., *Forum of Education*, III, 1925, pp. 186–94.

more convincing, because he was himself at one time so strong a critic of any theory which treated "intelligence" as more than a purely statistical "factor", i.e. a simple average of a person's performances in a number of different tests. In conjunction with Dr. H. G. Sutherland he tested in two successive years nearly 2,000 pupils at elementary schools in the Isle of Wight, and over 800 boys at three institutions of the Grammar School type. At the former the correlations with size of family were — 0·15 for one year and — 0·22 for the next; at the latter the correlations were naturally lower, but, in view of the selection, quite consistent with those from the unselected samples.[1] The tests used, however, were group tests, requiring ability to read; and the "I.Q. categories" were given in terms of letters, not of numerical quotients. However, from the diagrams given, it seems possible to calculate an equivalent I.Q. on the Binet scale. Adopting the same methods of computation as for London children, I find a decline of 1·9 I.Q.—almost exactly the same figure as before.[2]

The most recent and most thorough survey would seem to be that carried out by Dr. Fraser Roberts and his collaborators in Bath.[3] Here every endeavour was made to secure a sample of school children representing a *complete* cross-section of the population. The group comprised all who were born between September 1, 1921, and August 31, 1925, and whose homes lay within the boundaries of the city on July 27, 1934. These numbered 3,401; and full data were obtained for all but 96. All were tested by the Otis group tests (advanced scale); and 1,271 were also tested in addition by the Binet tests. The latter enabled the results from the Otis tests (which are expressed in terms of an "Index of Brightness") to be converted into terms of the more familiar Binet I.Q.; and incidentally showed that the use of a verbal group test had apparently "introduced no extra association, but, on the contrary, being a less efficient measure of intelligence, if anything minimized it".

[1] Sutherland, H. E. G., and Thomson, G., *Brit. J. Psych.*, XVII, 1926, pp. 81–92. Later investigations with specially chosen homogeneous populations (children of Yorkshire coal-miners, and children whose parents had been killed in the war) were undertaken with a view to demonstrating that the negative correlations were attributable primarily to hereditary ability rather than to home environment: see Sutherland, *J. Educ. Psych.*, XX, 1929, pp. 81 *et seq.*; id., *J. Genet. Psych.*, XXXVIII, 1930, pp. 161 *et seq.*
[2] Professor Thomson (in personal correspondence) has been good enough to express agreement with the mode of calculation adopted.
[3] Fraser Roberts, J. A., Norman, R. M., and Griffiths, Ruth, "Studies on a Child Population, III", *Annals of Eugenics*, VIII, 1938, pp. 178–215; cf. "Intelligence and Family Size", *Eugenics Review*, XXX, 1939, pp. 237–48. The survey was carried out under the auspices of the Burden Mental Research Trust, which owes its existence to the generosity of Mrs. Burden, of Clevedon Hall, Somerset.

The correlation between intelligence and size of family, measured by the number of each child's "living full sibs", proved to be — 0·224. The average size of family increased from 2·0, for an Index of Brightness of 210, to 4·8, for an Index of Brightness of 30; dropping, however, to 4·0, for an Index of 10. In terms of the Otis scale, the mean intelligence for all *children* was 99·8; the mean intelligence for all *families* (i.e. the mean which would be obtained if all families were of equal size) was 103·9. This indicates an average decline of 4·1 points per generation on the Otis scale. If we translate this into terms of the Binet scale, we obtain a decline of 1·6 or 2·0 I.Q. points, according as we employ the regression of the Binet scale on the Otis or the Otis on the Binet; and once again we reach an average figure remarkably close to those furnished by previous investigations.

Three special groups were selected from the whole sample. These were composed of the brightest, the dullest, and the average. Among both the average and the dullest there was a clear association (— 0·21 and — 0·24) between father's occupation and size of family. Here, therefore, even within a fixed level of intelligence, there is still a definite correlation between social class and fertility. Among the brightest, however, no such correlation was discernible. Consequently, assuming the small sample of 140 to be typical, we may reasonably infer that intelligent families from the poorer or lowlier classes must be just as infertile as among the higher social classes. However, as Dr. Fraser Roberts points out, these further analyses serve mainly to indicate how extremely complex the whole problem of differential fertility really is.

(7)

Alleged Increase of Mental Deficiency

The foregoing estimates have been reached by what I have called the indirect method. For the country as a whole the nearest approach to direct evidence is to be obtained from a comparison of the figures for mental deficiency reported by the Royal Commission on the Care and Control of the Feebleminded in 1907 and by the Joint Committee on Mental Deficiency in 1929. As a member of the latter committee, I may be allowed to comment on some of the data. I was asked to draft the standards and borderlines used in the committee's investigations; and the scale of intelligence tests adopted (my own revision of the Terman-Binet scale) was the same as that employed

in London.[1] Hence both the borderlines used and the figures obtained may be assumed to be comparable with those adopted for the school surveys in London described above. From his inquiries in 3 urban and 3 rural areas, Dr. Lewis (medical investigator for the committee) calculated that over the whole of England and Wales the incidence of mental deficiency in 1925 amounted to 8 per 1,000—6·7 in urban and 10·5 in rural areas (*loc cit.*, p. 82). From inquiries carried out in 1904–7, the Royal Commission on the Care and Control of the Feebleminded had reported an incidence of only 4·6 per 1,000. It was the unanimous view of the members of the committee, and of Dr. Lewis himself, that, as he puts it, "the greater thoroughness of the ascertainment accounts for a large, if not the greater, part of the increase".

When appointed Psychologist to the London County Council in 1912, one of my first tasks was to study the methods at that time available for ascertaining cases of mental deficiency and to consider possible methods of improvement. The figures I then obtained make it possible to hazard a guess at the additional number that might have been detected had later methods of assessment been available. I estimate that, with methods like those adopted for the Joint Committee's investigation, the Royal Commission's figure would have been increased to about 5·1 per 1,000. But it must also be remembered that the increasing care given, especially during infancy, to the mentally deficient, must have favoured the survival of a larger proportion at the time of the later survey: a comparison of mortality rates suggests that, with the later rate of survival, the Commission's figures would have been raised to something like 6 per 1,000; and, taking areas corresponding to Dr. Lewis's, I calculate that the comparable figures would have been (very roughly) about 5·5 per 1,000 in urban and about 7·5 per 1,000 in rural areas. This implies a rate of increase amounting in (say) 20 years to about 20 per cent. in urban and 40 per cent. in rural areas. The difference between the urban and the rural rates is, as Dr. Lewis suggests, probably due in part to differences in fertility, but mainly due to the migration of better stocks to the towns during the period in question. The allowances and estimates are, of course, little more than plausible guesses. An increase of 20 per cent. in 20 years would have raised the figure for feebleminded children in London from 1·54 per cent. to about 1·85 per cent. Actually, as we have seen, it apparently rose to only 1·71 per cent. But, if we consider that the increase was doubtless greater among the ineducable grades, the two calculations agree reasonably well.

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[1] *Report*, Pt. II, Chap. 2, pp. 37 f. and Appendix B.

Influence of Qualities other than Intelligence

No psychologist would maintain that innate intelligence is the sole factor making for civic or industrial efficiency. Some may therefore be tempted to argue that the more fertile classes, though less intelligent, might possess other qualities of equal or even greater importance—e.g. superior physique, superior manual or practical aptitude, superior moral qualities, like courage, candour, or freedom from nervous breakdown. In reply three points may be briefly noted. (i) The popular notion that children and adults of supernormal intelligence (the so-called "geniuses") are by nature more neurotic or degenerate—lacking in physical health, emotional stability, or moral firmness—has been completely disproved by numerous psychological studies. (ii) Moral and emotional qualities, whether superior or inferior, appear to be far less dependent on innate constitution and heredity than on post-natal and environmental influences. (iii) If class differences in innate specialized abilities exist, they must unquestionably be far smaller in amount and importance than the differences in intelligence, as above defined: what is popularly regarded as a specialized talent or aptitude is really a resultant of both high general intelligence and some more specialized ability or interest.

In any case, it may be safely argued that innate intelligence is the main factor *limiting* efficiency in practically every sphere of modern life: if not a "sufficient" cause of success, it is at least a "necessary" cause, i.e. a *sine qua non*.

(8)

Final Conclusion

So far as the available evidence can afford any guide, therefore, I am inclined to hazard the guess that, during the past generation, the decline in the I.Q. has varied about 1·5 points in urban areas and about 2·0 points in rural areas. 1·5 points on the I.Q. scale would be equivalent to about 2 mental months at the age of 10—or 3 mental months for adults; 2·0 points would be equivalent to 4 mental months for adults. But, even with these conservative estimates, a decline of this order would have grave effects on the mental status of the population if at all prolonged.

This perhaps can best be illustrated if we suppose the same rate to continue until the end of the century—not because I imagine the

rate will remain constant, but because the rate is more clearly expressed if referred to a longer interval. Accordingly, I have tabulated the approximate results for pupils from special, ordinary and secondary schools.[1] This restriction omits ineducable defectives at one end of the scale and pupils of a public school type from the other. However, the former are largely pathological cases; and estimates for these would be somewhat precarious, especially as they often come from families which are far smaller than those of the feebleminded. For other reasons, it is almost as difficult to obtain plausible estimates for both numbers and I.Q.s of public school pupils. Rural areas differ still more widely in their average level. Some are above, most below, the level found in urban areas; but the data are again exceedingly inadequate. Hence for simplicity I have started with a single initial distribution, namely, that for the London area, which appears to be the best established.

TABLE III

Estimated Distribution of School Population with Declining I.Q.

Assumed Rate of Decline per Generation .		1·5	2·0	1·5	2·0
Approximate Type	I.Q.	1920	1950	1950	2000	2000
Scholarship .	> 130	1·8	1·4	1·3	0·8	0·6
Central school	115–130	12·2	10·3	9·8	7·6	6·7
Good average	100–115	35·1	33·4	32·4	29·9	27·2
Poor average.	85–100	37·5	38·6	39·3	40·5	41·3
Dull and backward	70–85	11·9	14·2	14·9	17·9	20·1
Feebleminded	< 70	1·5	2·1	2·3	3·3	4·1

It will be seen that, if the rate assumed continues, then in little over 50 years the number of pupils of "scholarship" ability would be approximately halved and the number of feebleminded almost doubled. These results to my mind form a far more serious consequence than the lowering of the general average by about 5 I.Q. points.

But, as the reader will have perceived, at almost every stage of the discussion the argument has been gravely hampered by serious weaknesses in the data. I venture to urge, therefore, that systematically planned inquiries should be carried out by trained psychologists, and that in the planning, as well as in the final analysis, they should be

[1] I should again remind the reader that terms like "secondary", "central", "scholarship" pupil are used with the meaning they had at the time the investigation was carried out,

assisted by statisticians familiar with the requirements of modern statistical procedures. In areas where fairly complete surveys have been already carried out, the inquiries might be repeated with the same tests and (so far as possible) with the same families. In addition to the surveys made in London, Bath, and Scotland, many education authorities have used intelligence-tests on an increasing scale; and the samples thus available for comparison must now be fairly numerous. But fresh surveys with newer modes of testing are urgently needed on other grounds. As a nation we should know our resources in mind-power as accurately as we do in man-power, iron, or coal. If, as I hold (p. 26), the rate of decline is comparatively small, its existence and amount can only be definitely established by surveys repeated at fairly long intervals. If, on the other hand, as Dr. Cattell has maintained, the figure is large, then it should be discoverable in a relatively short period of time. But, since such surveys can also fulfil more practical aims—determining the number of pupils who need special provision, discovering the educational needs of different districts, guiding the education committee in planning schools of various types—they should (if possible) be repeated pretty frequently, at least twice (I would suggest) in every generation, that is at least once in every ten or fifteen years.

Appendix I

ON THE MEANING OF INTELLIGENCE

Intelligence as a Technical Term. The fact that children of more intellectual parents do better in "tests of intelligence" than children of duller parents does not of itself prove that the differences so disclosed are due to inheritance rather than to differences of environment or opportunity. It is therefore essential to ask what it is that so-called "tests of intelligence" really measure.

Most of the current controversies[1] about the inheritability of "intelligence" arise from a confusion over the different meanings attached to the word. As a rule, those who have attempted to show that intelligence is not affected by inheritance to any great extent take the word to mean either intelligence in some broad and popular sense, or else intelligence as actually measured by tests of intelligence, which are admittedly imperfect instruments. In this paper, on the other hand, the term has been used with a more limited and technical connotation.

The word "intelligence" and its synonym "general ability" owe their currency among psychologists largely to Galton and Binet. Nor can there be any doubt that what these two pioneers had chiefly in mind, and were seeking to assess, was inborn or "natural" ability. Since subsequent research has so fully confirmed their main assumptions, it seems reasonable to employ the word as they employed it.

Intelligence as a General Factor. Early investigators, it is true, questioned the existence of anything that could be called *general* intellectual ability, on the ground that the correlations between tests of intellectual abilities seemed so low.[2] This, however, as is now universally agreed, was due chiefly to the fact that the groups tested had, as a rule, already been selected for general intellectual ability. All recent investigators find fairly large correlations, running up to 0·6 or more between assessments of cognitive ability of every kind. It fol-

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[1] That the views I have attributed in the text to the "majority of present-day psychologists" are not shared by every psychologist without exception is sufficiently shown by the criticisms contained in a book published earlier in the present year (J. Blackburn, *Psychology and the Social Pattern*, Kegan Paul, 1945; see esp. pp. 7–8, 61–78, and 86). Since similar doubts are still felt by certain social writers, it seems necessary to devote a few pages to their examination.

[2] On these grounds, Thorndike, originally the chief opponent to the "general factor theory", held that the mind was merely a "host of highly particularized and independent faculties" (*Educational Psychology*, 1903, p. 39). He has, however, since abandoned his opposition (*The Measurement of Intelligence*, p. 421).

lows that a test-measurement for intelligence is not merely an average (or a weighted average) of *independent* test-performances; it is an average of *closely related* test-performances. In this sense it is a "common factor". Nevertheless, a mere analysis of correlations cannot demonstrate that such a "factor" is anything more than an abstract, statistical concept.

Intelligence as an Inheritable Factor. There is now, however, overwhelming evidence to show that, of the individual differences revealed by tests of this "general factor", the greater amount is due to the individual's innate or inherited constitution. The main lines of argument may be briefly summarized as follows:[1] (1) The commoner forms of "mental deficiency" consist essentially of a deficiency in general intelligence, and these forms are demonstrably innate. (2) With the exception of the rarer clinical types, certifiable "mental deficiency" differs only in degree, not in kind, from the still commoner cases of irremediable "dullness"; and this dullness seems also to be handed on by inheritance, often in the same families as are producing the definitely defective. (3) At the other end of the scale, supernormal ability appears disproportionately common in members of certain families (not necessarily in the same homes). (4) The correlation between the tested intelligence of members of the same family is of the same order as the correlations between their heights or weights, and increases with the closeness of the family relationship: e.g. between identical twins it is as high as 0·86; between siblings it averages about 0·50 (rather less, viz. 0·35 to 0·45, when the apparent effects of similar cultural environment is partialled out); between first cousins, 0·30; between second cousins, 0·24. (5) The mental differences between individuals in the same economic class are far wider than the differences between the mental averages for different economic classes: occasionally, children brought up in the most unfavourable conditions may show extremely high I.Q.s; while children brought up under the best of conditions may be dull or even mentally defective. (6) Children of highly intellectual parents obtain proportionately high scores even in tests of a laboratory type, such as cannot possibly be affected by environmental advantages.[2] (7) Changes in

[1] The majority of textbooks confine themselves to evidence obtained by psychological investigators in America: this is admirably reviewed in Sandiford's *Foundations of Educational Psychology* (1938; cf. pp. 71–129 and refs.). But such evidence is naturally not altogether convincing to British critics. Accordingly, a review of relevant investigations in this country was included in a recent paper on "Ability and Income", *Brit. J. Educ. Psych.*, XIII, 1943, pp. 83–92.

[2] This was the approach adopted in the investigation carried out at Oxford (*Brit. J. Psych.*, III, 1909, esp. pp. 169–76). Its primary object was

environmental conditions, and changes in health or emotional attitude, make very little difference to the I.Q. *when properly assessed*: the most marked environmental changes rarely alter it by more than 5 to 8 points.[1] (8) To study cases where the environment is virtually the same for all, we can test children brought up from earliest infancy in residential institutions or foster-homes: under these conditions the children still show wide differences in tested intelligence, and its amount proves to be correlated with that of their relatives, the correlation itself varying with the closeness of the kinship: a striking case is the high intelligence often shown by illegitimate children of dull or poverty-stricken mothers, when the father (possibly a casual acquaintance) was of high intelligence. (9) To study cases where heredity is practically the same, we can test identical twins: the amount of resemblance between the intelligence of identical twins is 0·86 (almost as high as the correlation between two successive testings of the *same* individual). Even when the twins have been reared separately from early years, the correlation is still 0·77—far higher than that between brothers or sisters brought up in the same home.

In these and other respects the inheritability of tested intelligence is thus remarkably like that of stature. The measurable height of a child may be reduced by illness or malnutrition, particularly during infancy; but that does not obscure the fact that the main determining influence, especially when all are brought up in favourable conditions, is still the individual's inborn constitution. And the same holds good of the measurements obtained with intelligence-tests.

Definition of Intelligence. Accordingly, I think it safe to say that the vast majority of psychologists would now agree that, of the variance exhibited by measurements of the hypothetical factor underlying all cognitive tests, one half *at the very least* is attributable to "nature" as distinct from "nurture". Hence, in view of its evident importance, we need a special term to describe the "native" part of the "common

to show that intelligence was *both* a "general" factor *and* an "innate" factor. Critics of that early effort, however, like Dr. William Brown, concentrated rather on the first point, feeling no doubt that, if there was no cogent evidence for a general factor, then the question of its innateness did not arise. However, Dr. Brown has now entirely withdrawn his objections to the general factor theory (*Brit. J. Psych.*, XXIII, pp. 352–66). As regards the second point, therefore, I venture to suggest that the line of approach adopted in my original paper is still the most conclusive, though perhaps the least striking: it is applicable to *all* degrees of intelligence, whereas the more usual lines of argument (from the mental defectives, geniuses, twins, etc.), if standing alone, might fail to convince, because (it could be held) they are concerned with somewhat exceptional types.

[1] Cf. Burt, *Mental and Scholastic Tests*, pp. 152–7; Sandiford, *loc. cit.*, pp. 91–4 and refs.

factor". Since this native component corresponds precisely with what the testers of intelligence originally set out to measure, it seems appropriate in psychological discussions to keep their term intelligence for this particular component, and so to define intelligence as *innate, general, cognitive ability*.

As thus defined, intelligence is merely a distinguishable influence, not something that can be separated in actual fact, or measured in isolation. Our knowledge of it is therefore bound to be imperfect. But I would add that, even if we adopt the view of the more cautious writers, and insist that intelligence may be no more than a statistical construct, the foregoing arguments about the decline of the average level of this common factor would still hold good. However, I personally believe that there is further evidence of a non-statistical kind which enables us to give a more concrete interpretation to the term.

A Physiological Interpretation. The mental processes essentially required by intelligence tests involve the integration of a variety of perceptual and motor activities into a systematic whole: the more the processes tested depend on this integration, the closer they correlate with intelligence. Now the essential characteristic of the central nervous system is, in Sherrington's phrase, its "integrative action". Hence it is natural to suggest that the individual differences in integrative capacity revealed by the tests may be primarily due to differences in the neuronic architecture of the central nervous system. The histological study of the cortex shows that in feebleminded persons the neurons are (i) comparatively few in number, (ii) have comparatively few branches, (iii) are less systematically arranged. These are precisely the kind of structural differences that we should expect if the foregoing suggestions were true. We may plausibly suppose that, in normal members of the population, somewhat similar if less conspicuous variations in the cortical architecture are also present. Further, since any given kind of bodily tissue tends to be of much the same general quality in the same individual wherever it is found (e.g. muscle, hair, skin, etc.), we may expect that the nervous tissue would also have the same structural qualities throughout the individual's nervous system, or at any rate throughout his cerebral cortex, though, of course, with minor regional differences. Finally, being structural, these differences may readily be conceived as being largely, if not mainly, dependent on genetic factors, i.e. in popular language, as being inherited or at least innate.[1]

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[1] I believe this expresses the tacit assumption of most psychologists, though it is seldom stated explicitly. The nearest approach is to be found in Thorndike's recent "quantitative hypothesis", viz. the hypothesis that "quality of intellect depends on quantity of neural connections" (*The*

Alternative Definitions. (1) The best-known statement of the case against the hereditarian view is that of Bagley, who represents the self-styled school of "rational equalitarians". His first paper,[1] which virtually started the psychological controversy, left his critics with the impression that, in his opinion, "human mentality alone among all biological traits is not subject to biological variation"; and that differences alike in everyday efficiency and in efficiency at intelligence tests were due mainly, if not solely, to the physical, social, and educational advantages or disadvantages of the individual tested. Similar contentions are still frequently brought forward by medical and social writers who distrust the evidence of intelligence tests.

In his later replies,[2] however, Bagley points out that his original arguments were misunderstood. He makes it clear that he was adopting a very broad interpretation of the word intelligence and defines it as "the ability to control behaviour in the light of experience". His real view is (he says) that "general intelligence depends in part on physiological functions, and must therefore *in part* (his italics) be determined by original endowment or physical heredity"; but at the same time social conditions (e.g. "systematic schooling") are so overwhelming in their influence that they can "counteract" the minor differences resulting from original nature, and "level up," the inborn disadvantages. What he chiefly protests against is the notion that

Measurement of Intelligence, Chap. XV). As an adherent of the old associationist school, Thorndike identifies the qualitative superiority of intelligence with a superiority in "the number of possible associations the individual mind can make", and attributes this to the "number of possible contacts in the associative neurones". He refers in passing to the view that higher forms of intellectual activity may be dependent on "integration, that is, capacity for having the neurones act with reference to one another"; but considers it an open question whether such a capacity (if it exists at all) may not be independent of intelligence as he conceives it.

[1] Bagley, W. C., "Democracy and the I.Q.", incorporated as Chapter I in the volume cited below.

[2] Bagley, W. C., *Determinism in Education: Papers on the Relative Influence of Inherited and Acquired Traits in Determining Intelligence* (1925). Bagley cites my own investigations on the Binet scale as "the only thoroughgoing attempt to determine what schooling contributes to intelligence as determined by tests"; and repeatedly quotes my figures as showing that "not less than 54 per cent. of whatever it is that is measured as native intelligence turns out to be the result of experience and training" (*loc. cit.,* pp. 18, 75 f., 133 f.). What I claimed to show was that something like 54 per cent. of the variance revealed by the *original* Binet scale (as published in 1911) appeared to be attributable to differences in home-environment or schooling (in the original scale many of the tests, since dropped, were tests of reading, writing, arithmetic, familiarity with money-matters, and the like): even so, 33 per cent. was attributable to "native intelligence".

innate endowment "sets a limit to a child's educability". But, as a more recent critic has observed, "if education were as powerful as Bagley implies, how is it that dull and defective children in good homes—e.g. a feebleminded boy, born to a medically qualified University professor—are not levelled up by these social advantages to the performances of their brothers and sisters, who may perhaps win first-class honours?" [1]

(2) A second group of American critics, chiefly members of the "behaviourist school", have endeavoured to abolish all hypothetical concepts which cannot be "operationally" defined. These writers prefer to re-define intelligence as "what intelligence tests measure". [2] It is then not difficult to show that intelligence as thus defined may be appreciably affected by health, fatigue, social environment, and educational facilities. The main argument for the operational type of definition is drawn from the practice of physical science. Yet no physicist, I imagine, would press this principle to such extreme lengths. Suppose we have a machine originally devised to measure energy supplied from some source, and that the energy so supplied (W_0 say) cannot be measured directly; "operationally" we can only measure the energy delivered by the machine (W_1 say), and estimate the wastage due to friction (F). Would any physicist hesitate to estimate the amount of supplied energy by the equation $W_0 = W_1 - F_1$, or complain that this would entail an undesirable "hypothetical concept"?

It seems clear, then, that the arguments against the influence of heredity are largely due to a confusion between three things: (i) so-called "intelligence" as manifested in daily life; I would prefer to call this "general intellectual efficiency"; (ii) "intelligence" as measured by some one test (without adjustment for irrelevant factors); this I would call "tested intelligence"; (iii) the innate component in these

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[1] In America the controversy between the hereditarian and the environmental schools was revived for a time by the publication of work on the improvability of I.Q.s among very young children by a group of investigators working at Iowa under the lead of Dr. Stoddard. Their results, however, have not been confirmed by other American investigators; and nearly all who have discussed the inferences drawn have pointed out the obvious fallacies in the arguments (see *40th Yearbook*; I have reviewed Dr. Stoddard's work at greater length in *Occup. Psych.*, XIX, 1945, pp. 39–42). Accordingly, although the results reported may be most valuable in suggesting what benefits may accrue from the change in early environment, they can scarcely be regarded as affording valid evidence on the limitations set by innate constitution.

[2] Similarly, Blackburn claims that "all that has been shown is that intelligence tests measure the ability to answer intelligence test questions" (*loc. cit.*, p. 68).

manifestations, i.e. innate general intellectual efficiency: and I suggest that the term "intelligence", when used without qualification, should be understood to refer to this and to this alone. That, after all, corresponds with the usage of most psychologists.

It will be noted that the position which the two schools just mentioned set out to attack is the alleged identification of (ii) and (iii). This position, they assert, is that of "recognized psychologists who naïvely assume that the tests actually measure pure native ability unadulterated by the products of schooling".[1] But, to the best of my knowledge, no "recognized psychologist" has ever maintained that tests "actually" measure "pure" ability. Every psychologist admits that the I.Q.s furnished by intelligence tests, particularly when automatically accepted without check or correction, may be influenced in *some* degree by schooling and other environmental differences—influences which (if he is estimating intelligence in the sense of *native* ability) he regards as errors to be eliminated as carefully as possible before a child is classified as mentally defective.[2] The difference between the two opposing parties therefore really reduces to a question of relative importance or degree.

The Contribution of Environmental Influences to the Crude Test-Results. In this country, the doubts expressed by most psychological critics have taken a slightly different line. The majority appear to have accepted the definition of intelligence as an innate general factor; but a number have doubted the possibility of relying on tests or similar methods for measuring this innate or inheritable component. Two arguments are generally adduced, both, I believe, sufficiently disposed of by more recent evidence.

(1) Most tests of intelligence are verbal tests; such tests necessarily depend on knowledge of words, and knowledge of words must depend on the education received at home and at school: hence the differences revealed by intelligence tests must in the main be differences dependent on education. In this form the argument can be met by pointing out (*a*) that non-verbal tests of intelligence, though slower and less reliable, nevertheless lead to the same general conclusions; (*b*) that in dealing with individual cases no practical psychologist would rely on the uncorrected result of a verbal test for his final

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[1] Bagley, *loc. cit.*, p. 133. Similarly, Blackburn protests against the view that "intelligence tests measure innate intelligence *and nothing more*" (p. 68) —a view which has never for one moment been entertained by any competent psychologist.

[2] Dr. Blackburn appears to forget this routine principle when he declares that, as a result of the psychologist's reliance on verbal tests, "an illiterate mechanical genius may find himself incarcerated in a home for mental defectives" (*loc. cit.*, p. 8).

assessment of a child's intelligence, if he felt that the child might be handicapped by lack of verbal knowledge.

In answer to this the critics still maintain that, in whatever form the test is cast, verbal or non-verbal, every test that can be devised must still depend on some kind of acquired knowledge or skill. This ignores the fact that, although we can never entirely eliminate the element of acquisition, we can in most cases equalize it: e.g. the words chosen may be so simple that they are equally familiar to all the testees.

(2) Many critics have maintained that, even if we can equalize the effects of education, nevertheless ill-health, poor feeding, and the fatiguability of children from the poorer classes must impair their performances in tests of intelligence. This argument is fully refuted by the work of Shepherd Dawson among children in hospitals. He found (and his conclusion has been amply confirmed by other experimenters) that, provided neither the central nervous system nor the ductless glands have been affected, even chronic disease does not perceptibly handicap the children in tests of intelligence, as distinct from tests of educational attainments or skill.[1]

(3) The most important reservation is one which has not, I think, been generally noticed. Serious illness or prolonged ill-health occurring in very early infancy might, I believe, permanently impair the development of the central nervous system, and so produce much the same effects as are observable in undoubted cases of inherited dullness. Such causes are likely to be most frequent among the poorer families and lower social classes where infant mortality is high. At the same time, a study of case-histories drawn from such families makes me doubt whether the effects are common enough or serious enough to introduce any large distortion in the results of the systematic surveys described above.

Further, it may be noted that, even if accepted, criticisms of this kind would not necessarily tell solely in the direction which the critics assume: for on their hypothesis school-feeding, infantile welfare, and

[1] "Intelligence and Disease", *Medical Research Council Special Reports*, No. 162 (1931), pp. 4, 51. In improperly constructed or improperly applied tests of intelligence such factors may undoubtedly affect the child. The most obvious instances are to be found in group tests, where the child's score depends largely upon speed: in a properly constructed group-test the problems increase steadily in difficulty so that low marks are due, not to low speed, but to the relative difficulty of the later problems. Emotional or neurotic conditions may affect performances in individual tests, especially if the test is verbal. But the critics who emphasize this point overlook the fact that indications of emotional or fatigued conditions can generally be detected even in group-tests (e.g. the child answers a difficult problem but makes stupid mistakes with easier problems) and the trained psychologist would be alert to allow for or discard such results.

better methods of instructing the dull and backward should during the past generation have improved the ability of the less intelligent to answer the tests of intelligence; in spite of that, the number of failures has increased. However, it is plain that further research is needed on all such questions.

As to the main issue, I fancy that most psychologists, both in this country and in America, would now probably agree with the oft-quoted conclusion reached by Barbara Burks, after an impartial analysis of all the available evidence: "Home environment contributes about 17 per cent. of the variance in I.Q. (as actually tested); parental intelligence accounts for about 33 per cent.; and the total contribution of innate and inheritable factors is probably not far from 75 or 80 per cent."[1] Sandiford sums up the matter in a sentence: "With intelligence as measured by intelligence tests, the contribution of heredity is about four times as potent as that of environment."[2]

[1] *27th Yearbook*, 1928, p. 309. As regards the manner in which intelligence is inherited, there appears to be an increasing agreement with the view, suggested by the curves of distribution, that it is normally transmitted by multifactor inheritance, in much the same way as stature: a few rare cases of extreme pathological deficiency seem due to single genes, and the remaining variations to the combined action of many genes (some possibly dominant in one direction). It is commonly reckoned that, under ordinary conditions, about 90 per cent. of the variability in stature is attributable to heredity. Cf. Burt, C., *The Backward Child*, 1937, p. 442 and refs.; Fraser-Roberts, J. A., *Medical Genetics*, 1940, pp. 228, 236 f.

[2] *Foundations of Educational Psychology*, 1938, p. 95. One minor source of confusion should be emphasized here. The object of the researches just cited was to determine the accuracy of the commoner intelligence tests, each taken just as it stands; but the conclusions so reached should not be applied to assessments based on corrected results, obtained in many cases from repeated testing. In diagnosing a case of mental deficiency or in recommending pupils for scholarships, the psychologist never relies solely on the results of an intelligence test taken as they stand. The child may be unwell, fatigued, or emotionally disturbed on the day of the test; he may recently have done a similar test at school; or, in virtue of absence from school, or of unusual home circumstances, the routine test may be unsuitable. Accordingly, as explained above, the crude test-results were always submitted to the children's teachers, and checked in other ways. Thus the conclusions reported in the body of this paper were derived, not from unchecked mental ages or test-marks, but from the best available assessment for each child. The need for such occasional corrections forms one of the many reasons why such surveys can be carried out far more effectively by a psychologist who is himself a member of the Education Office staff than by a psychological investigator, however competent, planning an inquiry from outside. (The use of these checks in my own investigations seems to have been overlooked by many critics of this pamphlet. *Note added*, 1952.)

Appendix II

The views reached in the foregoing pages have been criticized from two opposite standpoints. A number of writers have objected that I have unduly minimized what they consider to be "the inevitable effects of differential fertility on the intelligence of the nation". Others have argued that, since the influence of environment is far greater than that of inherited differences, neither the test-results nor the inferences derived from them can be taken as relating to genuinely inborn characteristics: provided environmental conditions are improved, we shall witness (so they contend) not a decline but an increase in the intelligence of the general population. However, for the most part the arguments put forward by both sets of critics are purely speculative and often based merely on a misunderstanding.[1]

Since this pamphlet was first published, one important inquiry into the actual facts has been carried out; and this has been freely cited as demonstrating that "the fears about the diminution of national intelligence have now been proved baseless", or at any rate that "the methods proposed are quite unsuited to reveal such a decline even if it existed". A few words therefore [2] seem necessary in regard to the conclusions that can validly be drawn from the results obtained.

Twenty years ago, with the assistance of a grant from the International Examinations Inquiry Committee, the Scottish Research Council undertook a complete survey of a single age-group of Scottish children.[3] The primary purpose was similar to that of the earlier London surveys, namely, "to ascertain the distribution of measured intelligence" among Scottish children of either sex, with a view more particularly to ascertaining how far the average 'intelligence quotient' agreed with that reported in other areas, how wide was the scattering about the average, and what proportion of children fell into definite categories, such as that of mental deficiency. In 1947 it was resolved, after consultation with the Population Investigation Committee, to repeat the survey, with certain important additions, in order to verify the allegation that "the average intelligence of members of large families is less than that of members of small families", and, if possible, to throw light on the apparent causes.[4] As in the London surveys, the inquiry was based, first, on a broad investigation carried out by group tests, and secondly on a more intensive study by means of individual tests. In the earlier survey of 1932 no data had been procured in regard to size of families; but in the second survey the negative correlation reported by previous investigators was fully confirmed. With the group test the correlation between intelligence and size of family was — 0·28; with the individual test, it was — 0·32. This is in close accord with the final estimate which I had suggested (p. 16) for a complete age-group. At the same time the investigators endeavoured to compare the average figures obtained in 1947 with those obtained 15 years earlier. In

[1] The more important of these earlier criticisms were examined in the discussion appended to the third Occasional Paper in this series, *The Trend of National Intelligence*: with a Symposium by Professors Thomson, Burt, Carr Saunders, and Penrose (1947).

[2] I have dealt more fully with these arguments—medical, sociological, and educational—in *Population Studies*, I, pp. 177–186, the *British Journal of Sociology*, I, pp. 154–168, and the *British Journal of Educational Psychology*, pp. 55–61.

[3] *The Intelligence of Scottish Children*, 1933.

[4] *The Trend of Scottish Intelligence* (1949).

the group test the average score had risen from 34·5 in 1932 to 36·7 in 1947; in the individual test, the average I.Q. increased from 101·6 to 102·5.

There is, I think, nothing in these figures to conflict with what has been said in the preceding pages. Taken at their face value, they certainly seem to contradict the more alarmist views of those who had talked of "a galloping plunge towards intellectual bankruptcy". Whatever changes are going forward, they are evidently small, and by no means easy to establish. But this was precisely the conclusion drawn above from earlier investigations. And the investigators themselves did not for one moment suppose that their results would settle the problem raised.

Nevertheless, the critics say, how can one account for the actual rise? Before replying, let us first note how slight the increase really is—less than 1 per cent. on the I.Q. scale, a couple of marks in the group test; and then let us consider the conditions under which the tests were carried out. In London it was thought wiser not to use the same group test in successive surveys, but to keep the individual test the same, partly for the purpose of standardization. In the Scottish inquiry the opposite course was adopted. In the 1932 survey the individual testing was based on the Stanford Revision of the Binet scale; in the 1947 survey it was based on the Terman-Merrill Revision. In both surveys the same group test was used. Now, as several of the Scottish teachers have since observed, many of them guessed that something like the previous test would probably be adopted, which was accessible to all in the 1932 report; and some would naturally deem it only fair to their pupils to give them a little preliminary practice. In any case, whereas in 1932 psychological testing was still a novelty both to Scottish teachers and to Scottish children, by 1947 every teacher and many of the children had become familiar with such devices.

Now, as I have insisted above, even the most trustworthy tests of intelligence are liable to be influenced in some small measure by environmental conditions. And if, as our critics maintain, such conditions have a far greater influence than innate differences on the test-results actually obtained, then it is indeed surprising that the increase was not much larger. Nor must we forget that, under the conditions of the Scottish surveys, with the exception of 240 cases in the second inquiry that had to be rejected altogether, the test-results had to be taken as they stood: there was no opportunity (as in the London surveys) to submit them to the teachers, or to re-test cases where fatigue, familiarity, ill-health or other irrelevant factors might have affected the results.

Finally, let us note that, if, as suggested above, the rate of change is "a decline of 1·5 points in a generation at the outside, probably much less" (p. 24), one could hardly expect to detect it with any certainty by means of two surveys, separated by an interval of 15 years only, particularly when only a single age-group was tested. Indeed, the investigators themselves have insisted that their survey is to be regarded as merely the first of a series, and point out how many of the defects that have emerged can be corrected. When the inquiry is repeated after another twenty years, it will, they now agree, be preferable to "use exactly the same *individual* test", whereas "it is quite unnecessary to use the same *group* test". Meanwhile, the Research Council has unquestionably made an admirable start which might well be copied in other areas. Above all it has shown that such surveys are perfectly feasible and yield a valuable mass of suggestive information.

OCCASIONAL PAPERS ON EUGENICS